



PHILOSOPHICAL
TRANSACTIONS.

I. *Of the Methods of manifesting the Presence, and ascertaining the Quality, of small Quantities of Natural or Artificial Electricity.* By Mr. Tiberius Cavallo, F. R. S.

The Lecture founded by the late HENRY BAKER, Esq. F. R. S.

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THE persevering industry of many ingenious persons in the philosophical world, since the beginning of the present century, has gradually, and with rapid success, examined the effects, ascertained the properties, and investigated

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the laws of a science almost entirely unknown before. Various wonderful phænomena of nature have been explained, means have been discovered of avoiding some of their dangerous effects, and a greater insight into the general operations of nature has been derived, from the knowledge of so great and so extensive a power as is commonly understood under the name of *electricity*; yet, if instead of contemplating its more striking phænomena, *viz.* those which are apt to surprise more by the magnitude of their effects, than by the instruction which they may afford, we endeavour to examine the subject more closely, we shall find that our knowledge of electricity goes very little, if at all, beyond the superficial part of it. We know, for instance, that a piece of glass, or other electric, when rubbed, will produce that power which we call electricity, that the glass will communicate the acquired electricity to a piece of metal, that the piece of metal will retain that power in certain circumstances, and so on; but no person has shewn how that power is generated by the friction, or what prevents its passage through the substance of some particular bodies. It has been ascertained, that the air of most countries, and probably of the whole world, as well as the clouds, fogs, rains, &c. are almost always electrified; but we are ignorant of the office which this electricity can have in the great laboratory of nature; for surely so general and so active a power can hardly be intended by nature, merely to intimidate mankind now and then with the thunder and the lightning.

It appears, therefore, that those persons, who are now willing to distinguish themselves in this interesting branch of natural philosophy, ought to examine the electrical power not so much in its accumulated, as in its incipient state. Its first
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origin, or very beginning, ought to be investigated, it being afterwards very easy to understand its increase; for we may easily comprehend, how a great quantity of it may be accumulated from the often repeated additions of its smallest portions.

This truth having been understood by several philosophical persons, has induced them to contrive instruments necessary for the purpose, and to make numerous observations, which have undoubtedly promoted the knowledge of the subject; but a great deal still remains to be done, before we can attain to the knowledge of the object in view, *viz.* of the real nature, of the first origin, and of the general use of electricity. The instruments hitherto invented are still inadequate to the purpose, and the known methods of operating are not free from considerable objections. To examine the peculiar constructions, intended uses, properties, and defects, of those instruments, as well as methods of performing the experiments, is the principal object of the present Lecture, which I have the honour of delivering to this learned Society, and which may, perhaps, be of use to other experimenters in this branch of philosophy; since what is mentioned in the following pages is the result of long experience, and of consideration. Of experience, which has often contradicted the sanguine expectations of a pre-conceived theory; and of consideration, which has frequently demonstrated the absurdity or insufficiency of schemes which, at first sight, seemed very easy and plausible.

The late Mr. JOHN CANTON was, as far as I know, the first person who constructed an electrometer, or instrument capable of shewing the presence of what was then considered as a small quantity of electricity. This instrument consisted of two small balls of pith of elder or of cork, fastened to the

two extremities of a linen thread, the middle of which was fastened to an oblong wooden box, in which the thread and balls were kept, when not actually in use. It was with such an instrument that Mr. CANTON himself, Father BECCARIA, and others, ascertained the electricity generally existing in the air; and that Mr. RONAYNE discovered the constant electricity of fogs. But in the course of my experiments, having made frequent use of such electrometers, it naturally occurred to me, that in several cases, when the electrometer gave no signs of electricity, or at least not sufficient to ascertain its quality, the cause of it was the respectively large size of the instrument; for a small quantity of electricity being diffused through the box, thread, and balls of the electrometer, had not power sufficient to separate the balls, and of course to shew its presence. In consequence of this, I contracted the size of the electrometers to such a degree as could be affected by less than the tenth part of that quantity of electricity which was necessary to affect Mr. CANTON's electrometer. But in making the electrometers very short, the stiffness of the threads, which had been insignificant in a great extent, became now very considerable; hence, instead of fastening the balls to the two extremities of one piece of thread, I found it necessary to suspend each ball by a separate piece of thread, the upper part of which was formed into a loop, which moved in a ring of brass wire.

The electrometers, thus improved, were still subject to a very great imperfection, which was the twisting of the threads; to avoid which I substituted fine silver wire, instead of linen threads, which answered very well. However, in observing the electricity of the atmosphere, those electrometers appeared to labour under a considerable inconvenience, which was their
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being disturbed by the wind; for it frequently happened, that neither the existence, nor the quality, of the electricity could be ascertained on account of the wind agitating the instrument, and because the electricity being in small quantity would not remain on the electrometer a time sufficient to bring the instrument within doors, where it might be examined without any obstruction. In order to remove this imperfection, I inclosed the electrometer in a bottle, as it is described in the *Philosophical Transactions*, Vol. LXX. which construction has been found to answer remarkably well.

This bottle electrometer has been since altered by various persons; though, in my opinion, those alterations do not tend to improve it altogether. M. DE SAUSSURE, by altering the shape of the bottle, and depriving it of a neck, has rendered it capable of retaining the communicated electricity only for a very short time*; whereas, some of those electrometers, constructed on my original plan, have retained the communicated electricity for more than four hours.

Besides this, M. DE SAUSSURE, as well as some who make these electrometers for sale, have substituted pith balls, instead of the conical corks of the original plan, the latter of which are preferable for two reasons; *viz.* first, because the balls of pith are apt to adhere to each other, so that on communicating the electricity they often do not shew any repulsion at all; or if the communicated electricity be considerably great, the pith balls, after adhering to each other for some time, are at last separated with violence, so as to fly to the sides of the bottle, which frustrates the experiment; whereas the corks are not near so subject to this inconvenience; and, secondly, because the conical shape renders those corks capable of presenting a much

* SAUSSURE's *Voyage dans les Alpes*, Tom. II.

greater surface to each other, than if they were globular, their weights being the same in both cases; for it is very well known, that, of all the solid figures, the sphere contains the greatest capacity under the least surface; and as the effects of electricity, in those cases, is proportionate to the surface, and not to the quantity of matter, it follows, that the conical shape is preferable to the globular.

Another alteration of the bottle electrometer was lately made by the Rev. Mr. BENNET, and is described in the Philosophical Transactions, Vol. LXXVII. Part I. It consists principally in substituting two slips of gold-leaf to the corks suspended by wires. This alteration has some peculiar advantages and disadvantages. Its advantages are in general a greater degree of sensibility, and a more easy construction. Its disadvantages are, first, that the instrument is not portable; and, secondly, that even when not carried about, it is apt to be spoiled very easily. However, in some cases it is very useful, so that, upon the whole, it may be considered as a very good improvement.

As the fastening of the slips of gold-leaf to this electrometer, and to let them hang parallel, is rather difficult, it will not be improper to describe a method which I have contrived, in order to accomplish it more easily. When the slips are cut, and are lying upon paper or leather, I make them equal in length, by measuring them with a pair of compasses, and cutting off a portion from the longest: then cut two bits of very fine paper, each about half an inch long, and a quarter of an inch broad, and stick one of them, by means of a little wax, which is rubbed upon them, to one extremity of each slip of gold-leaf, so as to form a kind of letter T. This done, I hold up, in the fingers of one hand, one piece of paper, with the gold leaf suspended to it, and hold the other with the other hand; then

then bring them near to each other, and after having adjusted them properly, so that the slips may hang parallel and smooth, I force the pieces of paper, which now touch each other, between the two sides of a sort of pincers made of brass wire, which are fastened to the under part of that piece which forms the top of the electrometer: and, in order to change those slips when spoiled, I keep in a book several slips of gold-leaf ready cut, and furnished each with a piece of paper; so that by this means this electrometer is rendered, in a certain manner, portable.

Besides the way of ascertaining small quantities of electricity, by means of very delicate electrometers, two methods have been communicated to the philosophical world, by which such quantities of electricity may be rendered manifest, as could not be perceived by other means. The first of those methods is an invention of M. VOLTA, the apparatus for it being called the *Condenser of Electricity*, and is described in the Philosophical Transactions, Vol. LXXII. The second is a contrivance of the above-mentioned Mr. BENNET, who calls the apparatus *The Doubler of Electricity*. A description of it is inserted in the Philosophical Transactions, Vol. LXXVII.

M. VOLTA's condenser consists of a flat and smooth metal plate, furnished with an insulating handle, and a semi-conducting, or imperfectly insulating, plane. When one wishes to examine a weak electricity with this apparatus, as that of the air in calm and hot weather, which is not generally sensible to an electrometer, he must place the above-mentioned plate upon the semi-conducting plane, and a wire, or some other conducting substance, must be connected with the metal plate, and must be extended in the open air, so as to absorb its electricity; then, after a certain time, the metal plate must be separated

separated from the semi-conducting plane, and being presented to an electrometer will electrify it much more than if it had not been placed upon the above-mentioned plane.

The principle on which the action of this apparatus depends is, that the metal plate, whilst standing contiguous to the semi-conducting plane, will both absorb and retain a much greater quantity of electricity than it can either absorb or retain when separate, its capacity being increased in the former, and diminished in the latter case.

Whoever considers this apparatus, will easily find, that its office is not to manifest a small quantity of electricity, but to condense an expanded quantity of electricity into a small space; hence, if by means of this apparatus one expected to render more manifest than it generally is, when communicated immediately to an electrometer, the electricity of a small tourmalin, or of a hair when rubbed, he would find himself mistaken.

It is Mr. BENNET's doubler that was intended to answer that end; *viz.* to multiply, by repeated doubling, a small, and otherwise unperceivable, quantity of electricity, till it became sufficient to affect an electrometer, to give sparks, &c. The merit of this invention is certainly considerable; but the use of it is far from precise and certain.

This apparatus consists of three brass plates, which we shall call A, B, and C; each of which is about three or four inches in diameter. The first plate A is placed upon the gold-leaf electrometer, or it may be supported horizontally by any other insulating stand, and its upper part only is thinly varnished. The second plate B is varnished on both sides, and is furnished with an insulating handle, which is fastened laterally to the edge of it. The third plate C is varnished on the under side only,

only, and is furnished with an insulating handle, which is perpendicular to its upper surface.

This apparatus is used in the following manner. The plate B being laid upon the plate A, the small quantity of electricity, which is required to be multiplied, is communicated to the under part of the plate A, and at the same time the upper part of B is touched with a finger; then the finger is first removed; the plate B is afterwards removed from over the plate A. The plate C is now laid upon B, and its upper surface is touched, for a short time, with a finger. By this operation it is clear, that if the electricity communicated to the plate A is positive, the plate B must have acquired a negative electricity, and the plate C must have acquired the positive, *viz.* the same of the plate A. Now the plate B, being separated from C, is laid as before upon A; the edge of C is brought into contact with the under part of the plate A, and at the same time the upper part of B is touched with a finger, by which means the plate B, being acted upon by the atmospheres of both the plates A and C, will acquire nearly twice as much electricity as it did the first time, and of course will render the plate C, when that is laid upon it, proportionably more electrified than before: thus, by repeating this operation, the electricity may be increased to any required degree.

The varnish on those surfaces of the plates which are to lie contiguous to each other serves to prevent the metal of one touching the metal of the other; for in that case, instead of one plate causing a contrary electricity in the other, the electricity of the first would be gradually communicated to the others, and would be dissipated.

As soon as I understood the principle of this contrivance, I hastened to construct such an apparatus, in order to try several

experiments of a very delicate nature, especially on animal bodies and vegetables, which could not have been attempted before, for want of a method of ascertaining exceedingly small quantities of electricity; but, after a great deal of trouble, and many experiments, I was at last forced to conclude, that the doubler of electricity is not an instrument to be depended upon, for this principal reason; *viz.* because it multiplies not only the electricity which is willingly communicated to it from the substance in question; but it multiplies also that electricity which in the course of the operation is almost unavoidably produced by accidental friction; or that quantity of electricity, however small it may be, which adheres to the plates in spite of every care and precaution.

Having found, that with a doubler constructed in the above described manner, after doubling or multiplying twenty or thirty times, it always became strongly electrified, though no electricity had been communicated to it before the operation, and though every endeavour of depriving it of any adhering electricity had been practised; I naturally attributed that electricity which appeared after repeatedly doubling, to some friction given to the varnish of the plates in the course of the operation. In order to avoid entirely this source of mistake, or at least of suspicion, I constructed three plates without the least varnish, and which, of course, could not touch each other, but were to stand only within about one-eighth of an inch of each other. To effect this, each plate stood vertical, and was supported by two glass sticks, which were covered with sealing-wax: see Tab. I. fig. 1. and 2., where AB is a wooden pedestal, $7\frac{1}{2}$ inches long, $2\frac{1}{2}$ broad, and $1\frac{1}{4}$ inch thick; C and D are the two glass sticks cemented into the stand or pedestal AB, and likewise into the piece of wood E, which is fastened to the back.

back of the plate. The plate itself is of strong tin, and measures about eight inches in diameter. The stand AB projects very little before the plate, by which means, when two of those plates are placed upon a table facing each other, the wooden stands will prevent their coming into actual contact, as may be clearly perceived in fig. 3.

I need not describe the manner of doubling or of multiplying with those plates; the operation being essentially the same as when the plates are constructed according to Mr. BENNET'S original plan, excepting that, instead of placing them one upon the other, mine are placed facing each other; and in performing the operation they are laid hold of by the wooden stand AB; so that no friction can take place either upon the glass legs, or upon any varnish; for these plates have no need of being varnished. Sometimes, instead of touching the plates themselves with the finger, I have fixed a piece of thin wire to the back of the plate, and have then applied the finger to the extremity of the wire, suspecting that some friction and some electricity might possibly be produced when the finger was applied in full contact to the plate itself.

It is evident, that as the plates do not come so near to each other in this, as they do in the other construction, the electricity of one of them cannot produce so great a quantity of the contrary electricity in the opposite plate; hence, in this construction, it will be necessary to continue the operation of doubling somewhat longer; but this disadvantage is more than repaid by the certainty of avoiding any friction.

Having constructed those plates, I thought that I might proceed to perform the intended experiments without any further obstruction; but in this I found myself quite mistaken: for, on trying to multiply with those new plates, and when no

electricity had been previously communicated to any of them, I found, that after doubling ten, fifteen, or at most twenty times, they became so full of electricity as to afford even sparks. All my endeavours to deprive them of electricity proved ineffectual. Neither exposing them, and especially the glass sticks, to the flame of burning paper, nor breathing upon them repeatedly, nor leaving them untouched for several days, and even for a whole month, during which time the plates remained connected with the ground by means of good conductors, nor any other precaution I could think of, was found capable of depriving them of every vestige of electricity; so that they might shew none after doubling ten, fifteen, or at most twenty times.

The electricity produced by them was not always of the same sort; for sometimes it was negative for two or three days together; at other times it was positive for two or three days more; and often it changed in every operation. This made me suspect, that possibly the beginning of that electricity was derived from my body, and being communicated by the finger to the plate that was first touched, was afterwards multiplied. In order to clear this suspicion, I actually tried those plates at different times, *viz.* before and after having walked a great deal, before and after dinner, &c. noting very accurately the quality of the electricity produced each time; but the effects seemed to be quite unconnected with the above-mentioned concomitant circumstances, which independence was further confirmed by observing that the electricity produced by the plates was of a fluctuating nature, even when, instead of touching the plates with the finger, they had been touched with a wire, which was connected with the ground, and which I managed by means of an insulating handle.

At last, after a great variety of experiments, which it is unnecessary to describe, I became fully convinced, that those plates did always retain a small quantity of electricity, perhaps of that sort with which they had been last electrified, and of which it was almost impossible to deprive them. The various quality of the electricity produced was owing to this, *viz.* that as one of those plates was possessed of a small quantity of positive electricity, and another was possessed of the negative electricity, that plate which happened to be the most powerful, occasioned a contrary electricity in the other plate, and finally produced an accumulation of that particular sort of electricity.

Those observations evidently shew, that no precise result can be obtained from the use of those plates, and of course that, when constructed according to the original plan, they are still more equivocal, because they admit of more sources of mistake.

As those plates, after doubling or multiplying only four or five times, shew no signs of electricity, none having been communicated to them before, I imagined that they might be useful so far only, *viz.* that when a small quantity of electricity is communicated to any of them in the course of some experiment, one might multiply it with safety four or five times, which would even be of advantage in various cases; but in this also my expectations were disappointed, as will appear from the following pages.

Having observed, after many experiments, that, *ceteris paribus*, when I began to multiply from a certain plate, for instance A, the electricity which resulted was generally positive; and when I began with another plate B, *viz.* considered this plate B as the first plate, the resulting electricity was generally
negative;

negative; I communicated some negative electricity to the plate A, with a view of destroying its inherent positive electricity. This plate A being now electrified negatively, but so weakly as just to affect an electrometer, I began doubling; but after having doubled three or four times, I found, by the help of an electrometer, that the communicated negative electricity in the plate was diminished instead of being increased; so that sometimes it vanished entirely, though by continuing the operation it often began to increase again, after a certain period. This shews, that the quantity of electricity, which however small it may be, remains in a manner fastened to the plates, will help either to increase or to diminish the accumulation or multiplication of the communicated electricity, according as it happens to be of the same, or of a different nature.

After all the above-mentioned experiments made with those doubling or multiplying plates, we may come to the following conclusion, *viz.* that the invention is very ingenious, but their use is by no means to be depended upon. It is to be wished, that they may be improved, so as to obviate the weighty objections that have been mentioned in the preceding pages, the first desideratum being to construct a set of such plates as, when no electricity is communicated, they will produce none after having performed the operation of doubling for a certain number of times.

Upon the whole, the methods by which small quantities of electricity may be ascertained with precision are, as far as I know, only three. If the absolute quantity of electricity be small and pretty well condensed, as that produced by a small tourmalin when heated, or by a hair when rubbed, the only effectual method of manifesting its presence, and ascertaining its quality, is to communicate it immediately to a very delicate electrometer,

electrometer, *viz.* a very light one, that has no great extent of metallic or of other conducting substance; because if the small quantity of electricity that is communicated to it be expanded throughout a proportionably great surface, its elasticity, and of course its power of separating the corks of an electrometer, will be diminished in the same proportion.

The other case is, when one wants to ascertain the presence of a considerable quantity of electricity, which is dispersed or expanded into a great space, and is little condensed, like the constant electricity of the atmosphere in clear weather, or like the electricity which remains in a large Leyden phial after the first or second discharge.

To effect this, I use an apparatus, which in principle is nothing more than Mr. VOLTA's condenser; but with certain alterations, which render it less efficacious than in the original plan, but at the same time render it much less subject to equivocal results. I place two of the above described tin plates upon a table, facing each other, as shewn in fig. 3. and about one-eighth of an inch asunder. One of those plates, for instance A, is connected with the floor by means of a wire, and the other plate B is made to communicate, by any convenient means, with the electricity that is required to be collected. In this disposition the plate B, on account of the proximity of the other plate, will imbibe more electricity than if it stood far from it, the plate A in this case acting like the semi-conducting plane of M. VOLTA's condenser, though not with quite an equal effect, because the other plate B does not touch it; but yet, for the very same reason, this method is incomparably less subject to any equivocal result. When the plates have remained in the said situation for the time that may be judged necessary, the communication between the plate B, and the
conducting

conducting substance which conveyed the electricity to it, must be discontinued by means of a glass stick, or other insulating body; then the plate A is removed, and the plate B is presented to an electrometer, in order to ascertain the quality of the electricity; but if the electrometer be not affected by it, then the plate B is brought with its edge into contact with another very small plate, which stands upon a semi-conducting plane, after the manner of M. VOLTA's condenser*; which done, the small plate, being held by its insulating handle, is removed from the inferior plane, and is presented to the electrometer: and it frequently happens, that the small plate will affect the electrometer very sensibly, and quite sufficient for the purpose; whereas the large plate itself shewed no clear signs of electricity.

If it be asked, why I use the semi-conducting plane for this small plate, and not for the large one? the answer is, first, because the large semi-conducting plane is incomparably more difficult to be procured than the small one; and, secondly, because the small plane may be easily deprived of any accidental electricity which may adhere to it; but the large one is more difficultly rendered fit for the purpose, especially as the large plate ought in general to remain upon it a much longer time than the small plate is to remain upon its semi-conducting plane.

The third and last case is when the electricity to be ascertained is neither very considerable in quantity, nor much condensed; such is the electricity of the hair of certain animals, of the surface of chocolate when cooling, &c. In this case the best method is to apply a metal plate, furnished with an

* This small plate is nearly of the size of a shilling, and the semi-conducting plane is of wood covered with copal varnish.

insulating handle, like an electrophorus plate, to the electrified body, and to touch this plate with a finger for a short time whilst standing in that situation; which done, the plate is removed, and is brought near an electrometer; or its electricity may be communicated to the plate of a small condenser, as directed in the preceding case, which will render the electricity more conspicuous. It is evident, that in this case the metal plate will acquire the electricity contrary to that of the substance in question; but this answers the same purpose: for if the electricity of the plate be found to be positive, one must conclude, that the electricity of the body in question is negative, and contrariwise. In this operation, care must be had not to put the metal plate too near, or in full contact with the substance to be examined, lest the friction, likely to happen between the plate and the body, should produce some electricity, the origin of which might be attributed to other causes.

Having thus far described the surest methods of ascertaining the presence and quality of electricity, when its quantity or degree of condensation is small, I shall now beg leave to add some farther remarks on the subject of electricity in general, and which have been principally suggested by what has been mentioned in the preceding pages.

On the hypothesis of a single electric fluid, it is said, that every substance in nature, when not electrified, contains its proper share of electric fluid, which is proportionate to its bulk, or to some other of its properties; and it is generally believed, that this equal or proportionate distribution of electric fluid takes place with the greatest part of natural bodies. However, the fact is far from being so; and I may venture to assert that, strictly speaking, every substance is always electrified, *viz.* that every substance, and even the various parts of the same

body, contain at all times more or less electric fluid than that quantity of it which it ought to contain, in order to be in an electrical equilibrium with the bodies that surround it.

At first sight it may be thought quite immaterial to know, whether the electric fluid is dispersed in the just proportion among the various substances which are not looked upon as electrified, or whether it deviates in a small degree from that proportionate distribution; but it will hereafter appear, that one of those assertions will lead us to the explanation of an interesting phenomenon in electricity, whereas the other does not admit of it; besides, what is called small difference of the proportionate distribution, inasmuch as it does not affect our instruments, may be sufficient for several operations of nature, which it is our interest to investigate.

If we inquire what phenomena evince this altered distribution, or the actually electrified state of all bodies, the preceding observations will furnish some very unequivocal ones; especially that of the doubling plates made after my plan, which shewed to be electrified even after having remained untouched for a whole month, during which time they had been in communication with the ground; for if each of them had contained an equal share of electric fluid, the electric atmosphere of one of them could not possibly occasion a contrary electricity in the other, and consequently no accumulation of that power could have happened.

A great number of instances are related in books on the subject of electricity, and in the Philosophical Transactions, of pieces of glass, of sulphur, of sealing-wax, &c. having remained electrified so far as to affect an electrometer for months after they had been excited, or even touched; but the following experiment will shew, in a clearer manner, the great

length

length of time that a quantity of electricity will remain upon a body.

Having constructed a gold-leaf electrometer in the nicest manner I could, and which, on account of the non-conducting nature and construction of its upper part, could remain sensibly electrified for several hours together, I communicated some electricity to it, which caused the slips of gold-leaf to diverge with a certain angle; and as the electricity was gradually dissipated, the divergency diminished in the same proportion. Now, whilst this diminution of divergency was going on, I looked through a small telescope, and by means of a micrometer measured the chords of the angles of divergency, setting down the time elapsed between each pair of contiguous observations; and as the chord of the angle of divarication is in the direct simple proportion of the density of the electric fluid*, I could by this means know how much electric fluid was lost by the electrometer in a certain time, and of course what portion of the electricity first communicated to the electrometer still remained in it. Let us make the chord of the angle of divarication on first electrifying the electrometer, or rather when first observed, equal to 16; or let us conceive that quantity of electricity to be divisible into 16 equal parts.

I observed, that when the chord of the angle became equal to eight, the time elapsed between this and the first observation was one minute; when the chord became equal to four, the time elapsed between this point and the preceeding observation was 3' 30"; when the chord became equal to two, the time elapsed since the preceding observation was 17'; and when the chord became equal to one, the time elapsed since the

* This proposition was first ascertained by F. BECCARIA. See Philosophical Transactions, Vol. LVI.

preceding observation was one hour and a quarter; after which the electrometer remained sensibly electrified for a long time.

In repeating this experiment, the times elapsed between the corresponding observations did not follow strictly the same proportion of increase; nor did they increase regularly in the same experiments, which may be attributed in great measure to the inaccuracy in observing, and to the fluctuating state of the air; but it could be safely inferred from all the experiments, that the times required for the dispersion of the electricity were at least greater than the inverse duplicate proportion of the densities of the electricity remaining in the electrometer. And if we imagine, that they continue to diminish in the same proportion of increasing time, which is far from being an extravagant supposition, we shall find, by a very easy calculation, that about two years after the electrometer would still retain the hundredth part of the electricity communicated to it in the beginning of the experiment; and as we do not know how far a quantity of electricity is divisible, or to what extent it may be expanded, we may conclude with saying that, strictly speaking, the electrometer would remain electrified for many years.

It may be inferred from this, as well as from many other experiments, that the air, or in general any substance, is a more or less perfect conductor of electricity, according as the electricity which is to pass through it is more or less condensed; so that if a given quantity of electric fluid be communicated to a small brass ball, one may take it away by simply touching the ball with a finger; but if the same quantity of electric fluid be communicated to a surface of about 100 or 1000 square feet, the touching with the finger will hardly take away any part of it.

If it be asked, what power communicates the electricity, or originally disturbs the equilibrium of the natural quantity of electric fluid in the various bodies of the universe; we may answer, that the fluctuating electric state of the air, the passage of electrified clouds, the evaporation and condensation of fluids, and the friction arising from divers causes, are perpetually acting upon the electric fluid of all bodies, so as either to increase or diminish it, and that to a more considerable degree than is generally imagined.

I shall lastly conclude, with briefly proposing an explanation of the production of electricity by friction, which is dependent upon the above stated proposition, *viz.* that bodies are always electrified in some degree; and likewise upon the well known principle of the capacity of bodies for holding electric fluid being increased by the proximity of other bodies in certain circumstances.

It seems to me, that the cylinder of an electrical machine, like ABC in fig. 4. must always retain some electricity of the positive kind, though not equally dense in every part of its surface; therefore, when the part of it A is set contiguous to the rubber *fg*, it must induce a negative electricity in the rubber. Now, when by turning the cylinder, another part of it B (which suppose to have a less quantity of positive electricity than the preceding part A) comes quickly against the rubber; the rubber being already negative, and not being capable of losing that electricity very quickly, must induce a stronger positive electricity in the part B, which is now opposite to it; but this part B cannot become more positively electrified, unless it receives the electric fluid from some other body, and therefore some quantity of electric fluid passes from the lowest part of the rubber to the part B of the glass, which additional quantity of electric fluid is retained by the part B
only

only whilst it remains in contact with the rubber; for after that, its capacity being diminished, the electric fluid endeavours to escape from it. Thus we may conceive how every other part of the glass acquires the electric fluid, &c. and what is said of the cylinder of an electrical machine may, with proper changes, be applied to any other electric and its rubber.

It appears, therefore, that according to this theory, a part of the rubber, *viz.* that which the surface of the glass cylinder enters in turning round, must serve to furnish the electric fluid to the glass, and the upper part must be possessed of a negative electricity capable of inducing a positive electricity in the glass contiguous to it. In fact, this seems to be confirmed by the general practice and experience; for that rubber answers best for an usual electrical machine, which can easily conduct the electric fluid with its under part, and the upper part of which is more ready to acquire, and to retain, the negative electricity; hence the rubbers are generally furnished with amalgam below, and with a piece of silk above; hence also, if the cylinder of the machine be turned the contrary way, it will produce little or no electricity.

It often happens, that the part which conducts the fluid, and that which acquires the electricity contrary to that of the electric, are not so disposed in a rubber as the one above described; but it remains always true, that the rubber must be possessed of those two properties, *viz.* to conduct the electric fluid very readily in one or more parts, and to acquire, as well as retain, on other parts, an electricity contrary to that acquired by the electric that is to be rubbed with it.



